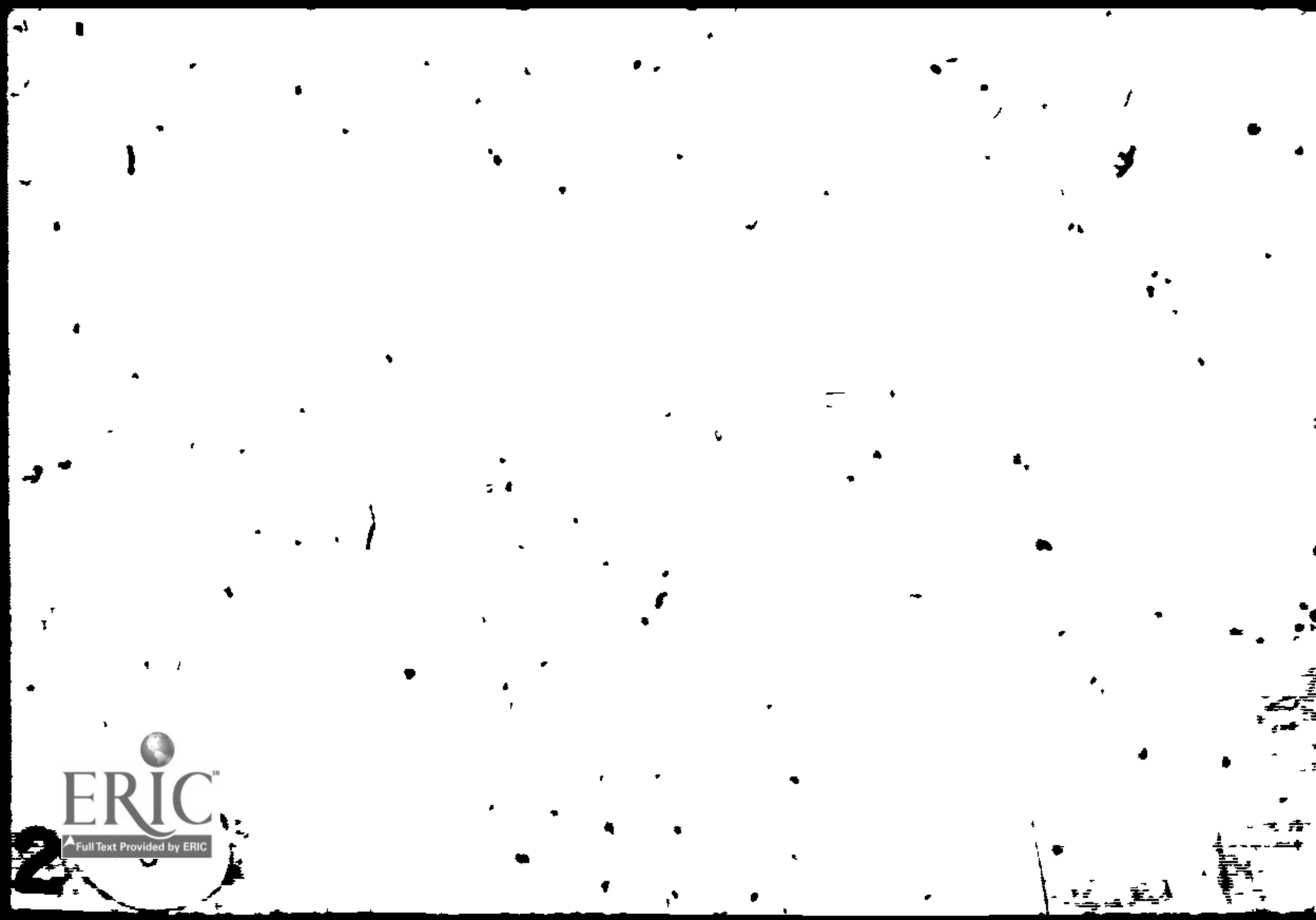

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ABSTRACT

The second Pittsburgh conference on the theory and practice of beginning reading, held in May 1976, is evaluated in this paper by pointing out some of the "critical incidents" that occurred at the conference and that indicated the types of interactions that must take place between reading theory and practice in the future. The paper suggests that by putting researchers in the classroom and by making theorists out of classroom teachers each group can appreciate the difficulties inherent in the other group's work. The interaction of such a situation will increase the volume of research--inadequate theories will be quickly discarded--and hasten the application of good theories into effective practice. (RL)

Old and New Routes from Theory to Practice

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Old and New Routes from Theory to Practice

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This conference was intended to foster translations from theory to educational practice. The "theory" side of the meeting deal mostly with psychological studies of information processing; the "practice" side dealt with the teaching of beginning reading. The conference was specialized. It sis not deal with all of the research that might be presumed to bear upon education nor did it deal with all of the subjects and goals contained within schools. But our discussions were directed towards a linkage between research and practice that ought to be important if any is. One kind of theory that education obviously needs is theory about how human learn. And one kind of practice that is central and archetypal in education is the teaching of beginning reading.

The conference went well. It was beautifully managed. The papers were substantive and interesting. It feel that I have learned a great deal from the presentations and could rest well content with the experience of this meeting. But, unhappily, it is my role as a discussant to seek out and bring up for discussion the central issues of the meeting. One of the things this leads one to do, in practice, is to look for small signs of tension or discomfort, in order to try to identify the topics and the issues that create excitement.

Let me quickly recall some events that seemed to me to be "critical incidents" during the conference:

--The first came when one of our speakers was outlining his recent research on word perception. The report was technical and detailed, because much current work on information-processing depends on rather refined experiments directed towards the differences among rather sophisticated theories. There was a break in the presentation for some discussion and Shirley Jackson interposed a question, "Why are you telling me this? What does any of this have to do with the classroom?" The question had an immediate impact. As soon as it was asked, you did suddenly wonder about what, exactly, teachers were supposed to get from a blow-by-blow portrayal of the technicalities of cognitive research. The question was aimed at a specific speaker but it went towards a major assumption governing the meeting as a whole. How do teachers and researchers benefit when each listens to a blow-by-blow account of the other's very different activities? Teacher-researcher conferences are common and commonly polite. Blunt, confronting questions are not often heard in the midst of them. Why not? The intimidation of the professional by the academic? The fear that, if the question is asked, some kind of bubble will burst?

--If the first critical incident led one to wonder about how meaningful theory is for practitioners, the second critical incident was even more direct and forceful. In Frank Smith's presentation, he said flatly that he has some real skepticism about whether theory can be translated into practice in any genuine way. He pointed out that attempts to translate theory into practice usually rest upon forced and oversimplified reasoning: "egregious overgeneralization", "the overlooking of important issues", and "the confusing of causes with consequences". It is quite common nowadays, as we all know, for researchers to proclaim that they have derived a principle

for education or, alternatively, for innovators in education to declare that their new procedures rest firmly on current research. Smith is, in effect, arguing that if we examine many of the advertised linkages between research and practice we will often find that they are unreal. The translation is ivalid on the research side, on the educational side, or both. Do researchers offer only convenient rhetoric for practitioners? Is there an alive and vital connection between research and practice in education?

--A third sort of perturbation arose twice during the conference, when instances were given of research work that seemingly had implications for education but that had been somehow ignored in practice. Dexter Fletcher spoke as the "ancient mariner", recounting his experience with the development of a computer-assisted method of instruction in beginning reading. That development program had achieved less than fond fantasy might have imagined at the beginning. But, nevertheless, it had some success and that reward it received for its success was that it was disbanded. There are not, apparently, continuations and explorations of the development effort in the practice of education. Why not? And Douglas Ellson presented his survey of innovations in education that had shown proven positive effects in evaluation studies. There have been many, many efforts to put forth innovative or compensatory educational programs in recent years. These programs have been evaluated and the general, easy, summary that most people carry around in their heads is that "Nothing Works". Yet Professor Ellson has carefully sifted through evaluation reprots and he finds a small body of studies wehre the evaluations were of acceptable scientific quality and where the data seem to say that something did, indeed, work. Now in these cases we seemingly have instances of research work that is not remoted

from education, off in the cloudlands of theory, but that consisted in demonstration and development efforts in educational practice. The work shows signs of success but, somehow, nobody seems to care. Why not? Isn't innovation in education a real issue? Isn't better education an issue?

--And now, fourth and last, we heard remarks that suggest that we may all be fiddling while Rome burns. Frank Smith in his talk stated that he has deep doubts about the ultimate consequence of the spectrum of efforts represented at the conference. He argued that if we were somehow to solve all of the problems that have been placed upon the conference table that might not make such differences for American literacy. To solve our national reading problems we might have to think about questions other than those of cognition, instructional design, classroom practice. We might have to address questions of social status, social distribution, politics and society. And Elsa Bartlett presented a disturbing analysis of current innovative curricula in beginning reading. Built into the educational innovations she sees a tracking structure, a higher level of instruction for the more well-off child and a lower level of instruction for the child of lower social status. Embedded in these two last remarks there appears to be a kind of "higher criticism" of the conference, arguing not that its methods are limited but that its goals are empty or even slightly mischievous. The more limited version of this higher criticism would assert that the technicalities of bettering reading instruction are largely irrelevant to the improvement of education. Teachers know how to teach reading and have been proving that for centuries. When they don't succeed, there are problems with access, not with method. A stronger version of the higher criticism would argue that the "reading problem" in American schools really arises out of poor social arrangements, perhaps as

a result of political arrangmenets that create a conspiracy of the haves against the have'nots. Conferences such as this one are diversionary. They seduce one into obsessing about technicalities rather than worrying about social injustice. They are part of the problem, not part of the answer. Science is the opiate of the scientist.

The above four items constitute my private list of the "critical incidents" arising during the meeting. They seemed critical because they were all denials, in smaller and larger ways, of the basic and optimistic premise on which the meeting was established--that basic research is relevant and helpful to education. In selecting these incidents, I have undoubtedly magnified them. After all, if individuals were really and totally persuaded that this kind of interchange is irrelevant or harmful it is doubtful that they would have consented to particiapte. But the incidents do bring to the surface doubts, doubts that are an undercurrent in many meetings of this type. It might be helpful to try to explore what they mean.

Older Assumptions of Educational Psychology

Dr. Marie Clay's address to the meeting was helpful in that, in two distinct ways, she set forth what one might call a developmental perspective embodied in her work. One important feature of a developmental perspective is the recognition that the work of the psychologist is directed towards organized systems. Organized systems maintain their integrity through a strategic balance of vital processes. They are not free to learn, adapt, or change in any old way. They can only modify their behaviors in some way consistent with that vital strategic balance (White, 1976). Dr. Clay noted at the outset of her remarks that if a psychologist approaches a group of teachers, she does not confront a group of people sitting there idly waiting

for some wise words. She confronts an organized educational system, teachers managing a complex body of educational routines and goals. The psychologist's advise and suggestions must be such that teachers can fit it in amongst all the other things they have to worry about. Then Dr. Clay noted a somewhat similar problem in the teaching of reading. Children have a functioning spoken language system. Part of the problem of teaching them reading lies in the fact that this preexisting organization interferes with the proposed new treatment of language. The teaching of reading must be such that children can fit it in amongst all the other things they have to worry about.

A somewhat analogous problem confronts us as a conferring group. Here we are in 1976, talking about the necessity of moving ideas from research into practice. Aren't we all aware that that's all been arranged for? There is a field called educational psychology, well represented in the American Psychological Association, organized separately in the American Educational Research Association. There are textbooks on educational psychology and, until very recently, every single teacher in this country had to pass a course in educational psychology in order to be certified. All this forms part of a system that is intended to move theory into practice.

Why are we sitting here trying this method of moving from theory into practice? We are here, in some way, expressing a belief that the established system is not complete in its scope or efficacy in 1976. Maybe the older system, established in the first decades of this century, is very roughly laid out. Maybe the older system is limited; it lays out one path from theory to practice but there is some need to consider other arrangements to provide more paths, arrangements such as this conference. We ought to consider briefly the nature of educational psychology as traditionally conceived. We may see

more clearly what our traditional lines of communication have been.

Educational psychology was established as a discipline in the first two decades of this century. Edward Thorndike published one volume on educational psychology in 1903 and a three volume series titled Educational Psychology in 1913-1914. His early work on learning and his later work on testing set the pattern for empiricism an ensuing discipline of educational psychology. The scientific figures who espoused or promoted the development of that educational psychology at around Thorndike's time are reasonably well known--Dewey, Hall, Cattell. The establishment of this educational psychology was not a simple fruition of scientific inquiry. It was a response evoked from the academic community to social movements that invited some specific kinds of cooperation.

Tyack's recent book, The One Best System, gives a history of American urban education in the last century with particular emphasis on the collaboration between social leaders and academics. The book describes the growth of a centralized professional "scientific" coordination of American education, which bound together the idiosyncratic educational practices of a set of small, locally managed, community-controlled school system. The idea of a "one best system" was the creation of reformers who sought to establish a more unified body of practice and standards in American education. American schools were in the hands of a local group of varying competence; professionalization would bring a uniform high standard based scientific knowledge and management. Tyack assigns a slogan to the movement, "the one best system", somewhat satirically. The movement had its positive and negative aspects: It addresses some real problems of the turn-of-the-century American school. Teachers were poorly paid, poorly educated, had low status. (Read Thorndike's

summary descriptions of American teachers and students in his Education: A First Book, published in 1912.) The 19th century American colleges were bewildering in their heterogeneity of admissions standards. Local schools were irregular in their facilities, curricula and standards. So a reform movement began that was in part stimulated by some genuine needs in the system, and that was in part quite conceivably a conspiracy of a WASP elite that sought to establish an overarching political control over schools that were more and more coming under the governance of local groups of ethnics. Political movements are like that, the holy and the unholy mixed together. The problems that precipitated the movement were "solved" and our conspicuous educational problems today are in important part problems of overstandardization--the overuse of standardized testing, the over-reliance on SAT scores for college admission, overstandardized management leading to manifold pressures for community relevance, community control, and pluralism in the organization of schooling.

People like Nicholas Murray Butler, Robert Thorndike, and G. Stanley Hall were active in the political movement described by Tyack--Butler extremely so. The movement had direct relevance for them. If education was to be scientifically managed, there had to be a source of that science somewhere. And so some leading universities began absorbing teachers' colleges or creating them, creating centers were research on education would take place and teachers would receive training based on the fruits of such research.

At the turn of the century, American psychologists were pretty much bottled up in the philosophy departments, growing more and more restless as their brass instruments carried their interests and theories further away from the normative philosophy of their time. Beginning in the 1890's

many Americans made an obligatory pilgrimage to Wundt's laboratory, and they brought back the procedures and the special concerns of all empirical epistemology. The imported German laboratory of Wundt and Titchener held sway for only a relatively short time in this country. Then wholly American trained psychologists (people like Thorndike and Watson) quickly established new trends. One of the things that enabled them to turn in new directions was the establishment of schools of education.

At the turn of the century there were hopes that psychology would lead to a scientific substrate for education, social work, child guidance, mental health and social progress in the broadest sense. These hopes fostered and conditioned the growth of psychology. Places and resources were given to psychologists who were concerned to lose the restrictions of the philosophy departments. The price they had to pay, of course, was that they had to give some kind of coherence and form to the general idea that there ought to be a scientific basis for education. And so we find Cattell, Thorndike, and Dewey in Nicholas Murray Butler's Teacher's College, setting the pattern for what was to become educational psychology. And at the same time, off on the side, we find G. Stanley Hall pulling together a Child Study movement. This movement, more of a coalition than a discipline, was short-lived. But something much like it came to life as the Child Development Movement of the 1930's, fading away and then reviving today to become the developmental-psychology that to impinges on education today.

The institutional form of that time that lasted and grew was the discipline educational psychology. The normative work of the discipline expressed a definite idea about the way theory moves into practice. The general idea was this: The scientist would sit in a laboratory, and he would discover

laws of learning. Teachers made aware of those laws should be more scientific and better teachers.

That's a vision that many of us still live with today, in slightly modified form. Most of us have abandoned the notion that there exist simple, general, widely applicable laws of learning that can be taught to teachers for the benefit of their practice. The behavior modifiers still believe this, and practice it. More people on both sides of the line should worry about the limited success that the behavior modifiers have achieved with their strategy. But most of us believe that we are going to need more than a few principles of shaping and rate-modification to explain learning and so we say don't ask us now, but sooner or later we are going to come up with laws and principles that will help us in schools. We can help you to think in different ways about learning, children, schools, development, socialization, motivation, behavior problems, etc. We can give you new ideas and cause you to reflect on your old ideas. But we are still at the drawing board, still building theories of cognition and learning. Where we have the science, we'll apply it.

What If We Apply Scientists Rather Than Science?

The older educational psychology is an organized system. It has institutional structure. There are jobs, journals, building, a professional society, book publishers, test publishers...much social paraphernalia organized around or adapted to a recognized entity. And that entity embodies the central idea that theory is made in universities and delivered to teachers to be embodied in practice. The idea is old but not completely inflexible. If we began in the early 1900's with the idea that researchers would make learning theory and norm-referenced tests, we can modify that premise to believe

that researchers should make information-processing theory or psycholinguistic theory or developmental theory or stage-reference tests. We can make changes within the traditional system while maintaining its integrity as a system providing that the changes are not too "radical". What is a radical change? A radical change, I think, is one that challenges the th[redacted] com-munication envisaged in and expressed by the institutional form of the system. The rather strong questions I noted at the beginning of this talk arise out of a relatively radical feature of recent communications between theory and practice. Instead of working through "channels"--sitting within the ivory tower and passing papers out to those who live in applied settings--researchers have taken to talking out of the ivory tower, entering as participants in the applied settings.

We seem to watch people walking out of the laboratories and standing in the schools, and working beside the practitioners. We find this in education, we find this in preschools, we find this in hospitals. And I suspect that this is a movement that is going to grow in the years to come. There is a movement not of science, but of scientists, between the laboratory and applied settings.

Notice that we have a number of people here who have moved back and forth between the two ecologies. And notice also that a lot of the discussions we had at this conference really drew upon the transitional experiences that people have had in the course of that movement. We found the special issues expressed in Dexter Fletcher's talk, Marie Clay's, Carol Chomsky's and Doris Johnson's.

Dexter Fletcher expressed well the gaps in theory that become apparent when one tries to rationally specify a computer system of instruction. He

talked about immense amounts of improvisation and guessing, about quick judgments on issues that might take a century or so to settle properly. There is no safe and solid way to move from theory to practice. He gave as well a good sense of the practical compromises that go on when you try to do something in the real world. We, in the laboratory tend to address ourselves mostly to basic principles, ideals, and relationships. But it's really rather surprising how much of practical intervention gets conditioned by small things, like the programming quirks of a 1500 or a PDP-10. What Dexter said, in short, was that you can't address an applied setting without some willingness to transcend the state of your own art and without making some generous concessions to the art of the possible.

Marie Clay said that if you continuously interact with an educational system, if you try to bring your point of view to the system, you are addressing a dynamic structure that has laws and constraints of its own, you have to study that structure and understand it in order to know how to act on it. We generally conceive of an applied scientist as a person who is scholarly about science, and concerned about practice. We rarely concede that an applied scientist might have to be a scholar about practice. There is much to be known about schools; they are much more complex structures than they appear to be on the surface (White, 1977). And many academic proposals and prescriptions for the betterment of education fail because they are based on simplistic stereotypes; they are irrelevant, incompetent and material with respect to the complex reality. (Anyone who thinks that defining "good education" or "better education" is easy might try asking the next ten people he meets to defint the notion.) By living within the functioning educational system, the sensitive researcher passes beyond the legendary and the stereotypic. The researcher

begins to see what the educator sees. And conversely.

Marie Clay said that a significant value of the scientist is in "perceptual training of the teacher". She talked about the value of the scientist not so much in offering theoretical truths as in framing and organizing reality. Then Jerome Rosner, in a not unmixed appraisal of the practical value of researchers, said, "One nice thing about researchers, they can describe things fantastically well." Not so long ago I heard Gregory Anrig, chief state school officer in Massachusetts, appraise the value of the researcher in something like the following terms: "I don't necessarily need people who can bring me answers. I can use people who bring me questions."

So researchers and practitioners working with each other begin to see things the way the other does and they begin to think like each other. At another conference, on the relation of science to policymaking, I once heard Wolfgang Edelstein of the Max Planck Institute of Education offer a symbolic interactionist view of the transmission between scientist and policymaker. The fundamentally productive aspect of the relationship, he argued, lies in what Freudians might call the transference relationship, or what George Herbert Mead might call the identification with the other. The scientist becomes a bit of a practitioner and the practitioner becomes a bit of a scientist. Now here, in this meeting, a similar argument has emerged repeatedly. Maybe this is the ultimate reason why we have begun to send scientists rather than science into the arenas of practice. Inforamtion is not enough. Maybe we need a theory of identification-processing.

The talks of Carol Chomsky and Doris Johnson's illustrated a kind of emergent creativity that one can find in applied work. What Carol Chomsky found were new patterns of phenomena. She was trying to teach reading through

writing and she came upon invented spelling patterns which seemed to be regular and lawful, which showed order, and which in a sense begged to be figured out. What Doris Johnson discussed were striking dyslexic dissociations of visual, auditory, graphic, and linguistic processes. Such dissociations lead you to think about the nature of thought and knowledge in new and interesting ways.

For the researcher, the move to practice can often be a visit to another laboratory. New phenomena appear, new patterns of order in nature. The movement out of the laboratory need not be a move away from fundamental inquiry. It can be a movement towards it. Most psychologists are, I think, insufficiently aware of a longstanding argument in the history of psychology revolving about the question of whether laboratory work is sufficient for--some would say even necessary to--the scientific study of psychology. A distinguished lineage ranging from Wilhelm Wundt through Lev S. Vygotsky to Urie Bronfenbrenner in our own time have all argued that one cannot fully develop a science of psychology by experimental work in a laboratory. Part of what we know must be sought in natural settings. So conceivably the psychologists of today are not moving into educational settings solely for the sake of social welfare. They might be doing so for the sake of science.

Some Conflicts of Scientists in Practice

After the scientist has paid the costs and reaped the benefits of entry into practical intervention, some special conflicts arise. The scientist represents, by role definition, professional values of science. But he has assumed some additional responsibilities. The scientist has become, if you like, a little bit of an engineer and/or a little bit of a teacher. He adopts other values that must be balanced against his scientific values.

Discussion at the meeting has again touched upon sensitive questions of the ultimate practicality of practical work, of the relevance of relevant research. The questions are new questions that might not be likely to crop up in an ivory tower, but they are striking and salient when one tries to make something happen in schools.

You have worked on an educational project. Inevitably you have had to do this in a local setting. You can't work on the system as a whole so you set up a project in Gary or Peoria or Pittsburgh. You are ingenious or lucky. You get a strong positive effect of the intervention. You know it, the teachers know it, and, mirabile dictu, the formal evaluation says so--cases randomly assigned to treatment and control conditions, simple statistical effects with no messy higher-order interactions, differences so sizable that they seem practically as well as statistically significant. You submit your paperbound report to Washington, which is quite likely to reward your success by a refusal to continue funding the project, and which will in any case sooner or later proceed to forget that the project ever existed. Your final report will become part of what has been called the "fugitive literature". Some concern-d scholar, such as Douglas Ellson at this meeting, may fish it out of ERIC and he may ask why nobody has ever done anything about it. Those who hear the question will look a little confused---educators, professors, bureaucrats--sensing that it is a perfectly reasonable question, but it is a perfectly reasonable question, but it is not their question. It is somebody else's, or it ought to be somebody else's.

So you have a scientist who has made the commitment, to practice, who had gone out and tried to make a change and ultimately nothing seems to happen. What do you do? The problem you face is that the world of education

contains very few ways for transmitting innovations and significant internal mechanisms for resisting their introduction. This is the Achilles heel of the contract mechanism so much favored in Washington these days.

You can describe the local operation in a paperbound report with the fate that I have discussed. You can write journal articles, or you can write in some journals that go to teachers. But you can't write much, and you can't show much. Or you can hope somehow to connect with the market mechanism. Some kinds of developments that are appealing to publishers or to educational hardware manufacturers get picked up and get carried around the system this way.

This limited, uncertain path to educational usage puts some delicate questions to the scientist-innovator. When one invests years and time and effort on a development project, then you have to care. Do you drop the innovation, implicitly conceding it to be an empty gesture, or do you accept the task of "selling" it? You get now into the funny responsibility to assume capabilities that are outside the scope of research work. You either turn into a marketer or a Guru or a preacher, or you at least have to find ways to coordinate with such people.